SCIENCE

A WEEKLY JOURNAL DEVOTED TO THE ADVANCEMENT OF SCIENCE, PUBLISHING THE OFFICIAL NOTICES AND PROCEEDINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

FRIDAY, OCTOBER 4, 1907

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ON THE CHEMICAL CHARACTER OF THE PROCESS OF FERTILIZATION AND ITS BEARING UPON THE THEORY OF LIFE PHENOMENA[1]

I

THERE may be a difference of opinion as to whether or not it will ever be possible to produce living matter from inanimate; but I think we all agree that we can not well hope to succeed in making living matter artificially unless we have a clear conception of what living matter is. Living organisms have the peculiarity of developing and reproducing themselves autofomatically, and it is this automatic character of reproduction and development which differentiates them, for the time being, from machines made of inanimate matter. Hence the answer to the question of what living matter is will have to be an answer to the question what determines the phenomena of automatic development and reproduction. Since all life phenomena are ultimately purely chemical, the answer must consist in pointing out one or more series of definite chemical reactions, for which it can be proved that they are identical with the phenomena of development and self-perpetuation. It always seemed to me that the natural starting point for a search after this definite chemical reaction or series of reactions was the analysis of that process which causes the resting egg to develop into an embryo, namely, the process of fertilization.

[1] Address delivered at the International Zoological Congress at Boston, August 22, 1907.

II

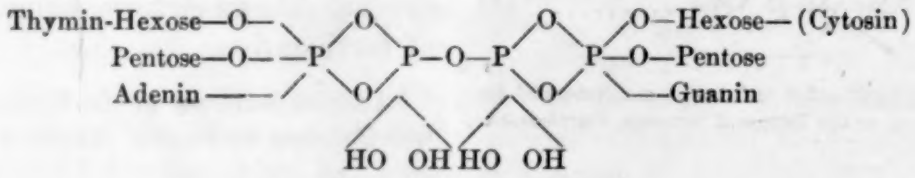
The spermatozoon produces two kinds of effects upon the egg. It causes the egg to develop and it transmits the paternal qualities to the offspring. We are here concerned only with the developmental effects of the spermatozoon. If the question be raised as to what is the most obvious chemical reaction which the spermatozoon causes in the egg, the answer must be, an enormous synthesis of chromatin or nuclear material from constituents of the cytoplasm. After the entrance of the spermatozoon the egg has one nucleus which during segmentation is successively divided into two, four, eight, sixteen, etc., nuclei. Boveri has shown that each new nucleus has the same size as the first nucleus after fertilization. It is therefore obvious that one, and, to all appearances, the foremost chemical effect of the spermatozoon upon the egg is an enormous synthesis of nuclear matter, and to this we must give our attention.

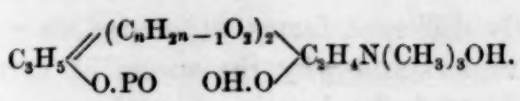
The main mass of the nucleus consists of a salt, the acid of which is nucleic acid, the base a protein substance of the type of protamins, the synthesis of which has been accomplished by A. E. Taylor, or histones. The skeleton of the nucleic acid molecule seems to be phosphoric acid, to which are coupled at least two chemical groups, namely, purin bases (adenin, guanin and possibly other representatives of the same group) and carbohydrates—a pentose and a hexose. The constitution of the nucleic acid may be represented by the following diagram by Burian, of which he states, however, that it can not be entirely correct since it has forty-one instead of forty atoms of carbon in the molecule.

From where does the material for this synthesis of nucleic acid after fertilization come? Our attention is first called to the phosphoric acid. In the case of eggs developing in sea-water, one might think of the possibility that these phosphates are taken from the sea-water. I have made experiments with artificial sea-water from chemically pure salts which contained no phosphates. Eggs of the sea-urchin accomplish their nuclein synthesis just as fast in solutions free from phosphates as in sea-water. Since the same is true for eggs caused to develop by chemical methods, it is obvious that the phosphates for the synthesis of nucleins are derived from the egg itself. The same is true for the other constitutents of the nucleus, since the segmentation of the egg of the sea-urchin can proceed to the blastula and gastrula stage in a solution containing only the chlorides of sodium, potassium, calcium and magnesium.

Miescher found that the amount of lecithin in the blood of the salmon is increased during the period of the formation of sex cells, and he concluded that lecithin is one of the substances from which nucleic acid is formed. The egg itself does not seem to contain any preformed nucleic acid, according to the investigation of Kossel, in the hen's egg, and of Tichomiroff in the eggs of insects. All eggs and possibly all embryonic cells possess comparatively large quantities of lecithin, a fact to which Hoppe Seyler has already called attention .

The lecithin consists of two different groups of bodies, one being distearyl (or oleyl), glycero-phosphoric acid, the other being cholin.





The cholin can apparently not be utilized for the synthesis of nucleins, but the other constituent is able not only to furnish the phosphoric acid skeleton of the nucleic acid molecule, but also the carbohydrates. The fatty acid could be rendered available for this purpose by oxidation, and we shall see indeed that phenomena of oxidation are the prerequisite of the synthesis of nucleins. If the lecithin of the egg is utilized for this synthesis it is obvious that the lecithin molecule must first undergo a cleavage, whereby it is freed from the cholin group. The question as to whether or not lecithin is the source of the phosphates and possibly some other constituents of the nucleic acid group can not be decided until the synthesis of the nucleic acid has been accomplished.

Some further data concerning the synthesis of nucleic acid in the egg can be obtained. The fertilized egg can not develop or increase the number of its nuclei unless an ample supply of free oxygen is present. Twelve years ago I showed that if all the oxygen is withdrawn from the egg, no nuclear, and no cell division is possible, and no increase in its mass of nuclein occurs. The same effect is accomplished if we inhibit the oxidations in the egg through the addition of KCN. The latter, or possibly the HCN formed by hydrolytic dissociation, inhibit the oxidations in the egg. The addition of $\frac{1}{2}$ c.c. of a 1/20 per cent. solution of KCN to 50 c.c. of sea-water is sufficient to bring to a complete standstill the effect of the fertilization in the egg of the sea-urchin, without otherwise injuring the egg, provided the latter does not remain too long in the absence of oxygen or the presence of KCN. As soon as oxygen is again admitted to the egg which has been deprived of it, the synthesis of nuclein and the segmentation begin again; the same occurs when the eggs are brought back from the cyanide sea-water to normal sea-water, provided they are sufficiently aired. It is therefore obvious that the nuclein synthesis depends absolutely upon a process of oxidation.

By way of digression I may mention that this rôle of oxidation in cell division should induce teachers of physiology to discontinue the antiquated statement that the sole object of oxidation in living cells is the production of heat. Oxidations occur in plants and animals which do not need to keep up a constant temperature. But in all these animals a synthesis of nuclein and cell divisions occurs.

We can further show that aside from the process of oxidation, still other processes are originated or accelerated through the entrance of the spermatozoon into the egg and that these processes occur even in the absence of oxygen. This follows from the difference in the resistance of the fertilized and the unfertilized eggs towards lack of oxygen. Fertilized eggs suffer much more quickly from lack of oxygen or from KCN than unfertilized eggs. Eggs were fertilized and a few minutes later put into sea-water free from oxygen, in which they remained twenty-four hours at a temperature of about 15° C. No segmentation or increase in nuclei occurred in the eggs while they were without oxygen. At the end of the twenty-four hours, air was admitted and segmentation began; many eggs, however, segmented abnormally and none developed beyond the blastula stage. Simultaneously the same experiment was made with the unfertilized eggs of the same female. When after twenty-four hours air was admitted and sperm added, all the eggs developed into normal plutei. The same experiment can be made more easily and strikingly by adding KCN to the sea-water. These facts show that lack of

oxygen or the suppression of oxidations in the egg injures the fertilized egg much more quickly than the unfertilized egg. This becomes easily intelligible under the assumption that the spermatozoon causes or accelerates still other processes in the egg than oxidations, *e. g.*, hydrolyses, and that the products of these hydrolyses are utilized in the processes of oxidation. If the oxidations do not occur the products of the hydrolyses accumulate in the egg or give rise to further reactions not compatible with the life of the egg. It thus becomes comprehensible why the fertilized egg suffers much more readily than the unfertilized egg, which seems to be able to stand the influence of lack of oxygen for several days, and possibly a longer period of time. It is of course possible that among the hydrolyses occurring in the fertilized egg might be those of lecithin.

III

Our present knowledge of the chemical structure of the spermatozoon does not enable us to state why the entrance of the spermatozoon into the egg should cause its development. That part of the spermatozoon which prevails by its mass is its head, which seems to have essentially the same chemical composition as the egg nucleus or any other cell nucleus. The tail of the spermatozoon is cytoplasm which is at present not characterized by anything special except a relatively large amount of lecithin and fat. If we wish to gain any further insight into the nature of the process of fertilization we must turn to those experiments in which the action of the spermatozoon on the egg can be imitated more or less completely by well-known chemical agencies. The data on heterogeneous hybridization seem to indicate that the substances which cause the egg to develop must be identical or closely related in widely different forms, otherwise we could not understand why the sperm of various starfish, of the brittle star, the crinoids, and, according to Kupelwieser, even of mollusks, should be able to fertilize the egg of the sea-urchin. It almost looks as if the only limitation to heterogeneous hybridization were the fact that for some reason the spermatozoon is not able to enter into the egg of a widely different family. This may explain why it is often necessary to change the constitution of the sea-water, for example, to raise its alkalinity, in order to enable the spermatozoon to enter the foreign egg. It follows from this fact that we can draw conclusions upon the nature of the process of fertilization only from such methods of artificial parthenogenesis as are of a more general application.

We shall begin our discussion with a consideration of the methods of artificial parthenogenesis in the sea-urchin, since they have been most thoroughly investigated in this form. The first method by which larvæ were produced from the unfertilized egg of the sea-urchin consisted in treating the eggs with sea-water whose osmotic pressure had been raised about 50 per cent. The method consisted simply in putting the unfertilized eggs for about two hours at a temperature of about 20° C. into a mixture of 50 c.c. sea-water plus $7\frac{1}{2}$ c.c. $2\frac{1}{2}N$ NaCl and then transferring them to normal sea-water. This method, which gave comparatively constant and good results on *Arbacia* at Woods Hole, gave unreliable results in a form of sea-urchin common at Pacific Grove, *Strongylocentrotus purpuratus*. Neither were the results obtained with this method, on the shore of France and at Naples, very satisfactory, according to reports by Giard, Herbst and others; while E. B. Wilson obtained good results with this method on *Toxopneustes* at Beaufort, North Carolina.

Whenever we notice such an inconstancy of results by a definite method it is probable that some essential variable of the experiment has been overlooked. It seemed difficult to suggest what this variable might be. But there was another chance of overcoming this block. I had noticed in my first experiments that the unfertilized eggs which had been caused to develop by the treatment with hypertonic sea-water differed typically in the form of their development from the eggs fertilized by sperm. This fact was at first welcome since it disposed of the general objection that my results were due to an infection by sperm. The main differences were the following: the egg fertilized by sperm forms immediately after the entrance of the spermatozoon a fertilization membrane, while in the egg caused to develop by the osmotic treatment no membrane was formed. It was also found that a segmentation and development of the egg occurred more rapidly and more regularly in the fertilized egg than in the egg treated osmotically. These and other differences led to the idea that the treatment of the egg with hypertonic sea-water initiated only certain, but not all of the developmental effects of the spermatozoon. It therefore seemed to be necessary to find a second agency which in combination with the osmotic treatment would allow a more complete imitation of the effects of the spermatozoon. In the pursuit of this idea it was found that if the unfertilized eggs of the Californian sea-urchin, *Strongylocentrotus purpuratus*, are treated for some minutes with sea-water to which a small but definite amount of a monobasic fatty acid (or any acid with only one carboxyl group) was added, they will form a typical membrane of fertilization after being transferred to normal sea-water. If these eggs are subsequently treated for from 30 to 50 minutes with hypertonic sea-water (50 c.c. sea-water plus 8 c.c. 2½*N* NaCl) at a temperature of 15° C., practically all the eggs will develop into larvæ, provided the time of exposure to the hypertonic sea-water is correctly chosen. In a part of these eggs the segmentation occurred in a perfectly normal way and these eggs developed into normal plutei. If the eggs are treated with only one of the two agencies, the fatty acid or the hypertonic sea-water (for from 30 to 50 minutes) no egg develops. The calling forth of the membrane formation alone leads, at room temperature, to the formation of the first nuclear spindle and division, and then soon to a rapid disintegration of the egg. The experiment can also be made in the reverse order, namely, the eggs can first be treated with the hypertonic sea-water and then be submitted to the treatment with fatty acid. If this order is adopted the eggs must be exposed to the hypertonic solution much longer than in the other case, namely, for from 1½ to 2 hours. This difference in the duration of exposure is due to the fact that the process of membrane formation leads to an acceleration of certain chemical reactions in the egg, whereby the hypertonic sea-water can accomplish its effects more quickly than if it is applied to an intact egg. The superiority of this new method of artificial parthenogenesis over the old one is very striking in the eggs of *Strongylocentrotus*. It happened often enough that the old, purely osmotic method of artificial parthenogenesis led to the formation of none or a small percentage of larvæ, while the new method of combining the fatty acid treatment with the osmotic treatment led to the development of the majority or practically all the eggs of the same female.

It could be shown that in this method we are not dealing with the direct effect of the fatty acid upon the egg, but with the effect of the membrane formation caused

by the fatty acid. To cause the membrane formation it was necessary to put the unfertilized eggs at 15° C. for from 1½ to 2½ minutes into a mixture of 50 c.c. sea-water plus 2.8 c.c. *N*/10 butyric acid (or any other monobasic fatty acid). If the eggs were taken a little too early from the solution, after about eighty seconds, only part of the eggs formed membranes after being transferred to normal sea-water. If these eggs were afterwards treated for from 30 to 50 minutes with hypertonic sea-water, only those eggs developed into larvæ which had formed the membrane. A further proof lies in the following data. In 1887 O. and R. Hertwig published the fact that if chloroform is dissolved in sea-water the sea-urchin eggs may form membranes in such sea-water, and Herbst showed in 1893 that benzol, toluol, xylol, act in the same way. I suspected that all fat solvents have the same effect, and an experiment with amylen confirmed this expectation. If the membrane formation is called forth in the egg of *Strongylocentrotus purpuratus* with any of these fat solvents and the eggs are afterwards treated for from 30 to 50 minutes with hypertonic sea-water they will develop into larvæ. It is, however, important to realize that these fat solvents cause cytolysis of the eggs, unless the latter are transferred very rapidly to normal sea-water. On account of this cytolytic effect it is preferable for practical purposes to cause the membrane formation by a fatty acid.

It is therefore obvious that we are now in possession of a method which allows us to imitate more completely the effects of the spermatozoon than the previous purely osmotic method. My attention was again directed towards the fact that the purely osmotic method gave unreliable results with the eggs of the Californian *Strongylocentrotus*, while it gave better results with the eggs of the eastern *Arbacia*. Experiments on the effect of the alkalinity of the sea-water upon segmentation indicated that the sea-water in the laboratory at Woods Hole is considerably more alkaline than that used by me in Pacific Grove. It occurred to me whether this difference might have something to do with the difference in the result of the osmotic method in both places. This suggestion proved correct. It was found that a neutral hypertonic solution with a concentration of hydroxyl ions of 10^{-7} or 10^{-6} normal, as a rule does not cause the development of the unfertilized egg of *Strongylocentrotus*, no matter how high the osmotic pressure is; but that with a sufficiently high concentration of hydroxyl ions a comparatively small increase in the osmotic pressure of the sea-water is sufficient to cause the unfertilized eggs of *Strongylocentrotus* to develop into larvæ. It was, moreover, found that the minimal concentration of hydroxyl ions in the hypertonic solution necessary to call forth the development of unfertilized eggs differs considerably for the eggs of different females. For the eggs of some females this minimal concentration was as low as that found in the sea-water at Pacific Grove, namely, between 10^{-6} and 10^{-5} normal, but as a rule a higher concentration of hydroxyl ions was required. It is possible to obtain good and constant results with the purely osmotic method in *Strongylocentrotus* if only the concentration of the hydroxyl ions in the hypertonic solution is sufficiently raised through the addition of NaHO. In cases in which the eggs of *Strongylocentrotus* will not develop into larvæ when put for about two hours at 15° C. into a mixture of 50 c.c. sea-water plus 8 c.c. 2½*N* NaCl they will develop when about 1.5 c.c. *N*/10 NaHO is added to this solution.

The apparently purely osmotic method, therefore, turns out to be composed of two agencies, one being the increased osmotic

pressure of the solution, and the other the concentration of the hydroxyl ions. It could be shown that these two agencies can with good results be applied separately, and that therefore there exists a far-reaching analogy between the effects of the alkali in these experiments and the fatty acid in the experiments previously mentioned. If the unfertilized eggs of *Strongylocentrotus* are first put for two hours into a mixture of 50 c.c. of a neutral van't Hoff solution isotonic with sea-water plus $\frac{1}{2}$ or 1 c.c. $N/10$ NaHO and then for from 30 to 50 minutes into hypertonic sea-water (50 c.c. sea-water plus 8 c.c. $2\frac{1}{2}N$ NaCl) many eggs or the majority will develop into larvæ. If the eggs are treated with the alkaline solution alone, without being subsequently treated with hypertonic sea-water, they will not develop. The treatment of the eggs for from two to three hours with NaOH acts, therefore, in a way similar to the treatment of the same eggs for about two minutes with a solution of the fatty acid of the same concentration. The analogy shows itself also in that with this method of combining the effects of alkali and hypertonic sea-water those eggs which develop into larvæ form often, if not always, a membrane. This membrane is not quite as distinct as the fatty-acid membrane for the reason that it surrounds the cytoplasm more closely. This membrane formation does not occur or does as a rule not become manifest until the eggs are returned from the hypertonic to the normal sea-water. If the order of events is reversed and the eggs are first put into the hypertonic sea-water and afterwards into the hyperalkaline solution, they must remain longer in the hypertonic sea-water, namely, for from $1\frac{1}{2}$ to 2 hours; this also corresponds to the experience with the fatty-acid treatment.

We therefore possess two parallel methods by which we can imitate the fertilizing effects of the spermatozoon upon the egg of *Strongylocentrotus*, namely, we treat the unfertilized egg for from 1 to 2 minutes with a solution of fatty acid (50 c.c. $M/2$ van't Hoff solution plus 0.7 c.c. $N/10$ butyric acid) or for from 2 to 3 hours with an equivalent alkaline solution (50 c.c. $M/2$ van't Hoff solution plus 0.7 c.c. $N/10$ NaHO) and afterwards for from 30 to 50 minutes with hypertonic sea-water (temperature = 15° C.). The treatment of the eggs with fatty acid or alkali can be replaced by a treatment with a fat solvent. Fatty acids, alkalis and fat solvents all act in the same way, namely, by causing those changes in the egg which result in the process of a membrane formation.

IV

I have not yet had time to apply these results to the eggs of many other forms, but I believe from what I have seen that we are now in possession of at least some of the general methods and principles of artificial parthenogenesis. It seems that in general the treatment of the eggs with alkalis and acids, sometimes with, sometimes without subsequent treatment with hypertonic sea water, causes the development of unfertilized eggs.

The unfertilized eggs of *Polynoë*, a marine annelid, can develop into larvæ if they are permanently put into hyperalkaline sea-water, *e. g.*, 50 c.c. sea-water plus 1.5 c.c. $n/10$ NaHO. It is well to keep the eggs for this experiment in shallow watch-glasses which are loosely covered with glass plates; in this case the oxygen of the air diffuses more readily to the eggs, than if they are kept in dishes with a deep layer of liquid above them. These eggs of *Polynoë* are immature when taken from the ovary and do not become mature in ordinary sea-water unless they are fertilized by a spermatozoon. They become, however, mature without the aid of sperm,

when kept for some hours in hyperalkaline sea-water. They form a membrane and throw out the polar bodies. If we wait until this occurs and then transfer the eggs for from 2 to 3 hours at 15° C. into hypertonic sea-water (50 c.c. sea-water plus 8 c.c. $2\frac{1}{2}n$ NaCl) and after this time bring the eggs back into normal sea-water they develop more quickly and more eggs segment than if they remain permanently in the hyperalkaline sea-water without any treatment with hypertonic sea-water.

Three years ago I had found that a small number of the eggs of mollusks, *Lottia gigantea,* and various forms of *Acmæa,* can be caused to develop if put for 2 hours into hypertonic sea-water. I have convinced myself this year that no development occurs if they are treated with a neutral hypertonic solution; that, however, if the alkalinity of the hypertonic solution is raised sufficiently high by the addition of NaHO many, if not practically all, the eggs of *Lottia* can be caused to develop into larvæ. In these experiments it was also noticed that the concentration of the HO ions in the hypertonic solution necessary for the production of larvæ from the unfertilized eggs differed considerably for the eggs of different females.

I have convinced myself also that the unfertilized eggs of *Sipunculus* can be caused to develop into larvæ by putting them permanently into a solution with a comparatively high concentration of HO ions.

As far as the production of larvæ from unfertilized eggs with the aid of acids is concerned, we may mention the eggs of starfish, which can be caused to develop with the aid not only of the acids containing one carboxyl group, but apparently with the aid of all acids. They differ in this respect from the eggs of the sea-urchin. It is possible that the acids with one carboxyl group act also better in the case of the starfish egg than the other acids; and this might explain why Delage obtained better results with CO_2 than with other acids, although according to my own experience the results with the other acids are much more satisfactory than those of Delage.

For the eggs of the starfish the acid treatment suffices, and no further treatment with hypertonic sea-water is required. In the egg of one form of starfish, namely, *Asterina,* the spermatozoon causes a membrane formation which is just as distinct as in the sea-urchin egg. The membrane formation can be induced in *Asterina* by exactly the same methods as in the sea-urchin egg, namely, a treatment with a fat solvent (benzol, or amylen) or a fatty acid. In these eggs the production of the membrane is sufficient to cause the development into normal larvæ at least of a number of eggs and an after-treatment with hypertonic sea-water is not required. The starfish eggs differ also from the sea-urchin eggs in the former having a tendency to develop spontaneously if left in sea-water, although the number of eggs developing in this way is, as a rule, very small. This development may be due to the action of the HO ions in the sea-water, or the action of an acid, *e. g.,* CO_2, formed in the egg itself. In *Asterina* it can also be noticed that if eggs remain in sea-water occasionally some of them form a membrane spontaneously, possibly also through the influence of the HO ions of the sea-water or an acid formed in the egg.

In the eggs of *Thalassema mellita,* a marine worm, Lefevre has produced membrane formation and normal segmentation by treating them with acid. The eggs of this form are immature when removed from the ovary, and the entrance of the spermatozoon causes them to form a membrane, to throw out their polar bodies, and to segment and develop. Lefevre found

that by treating the unfertilized eggs with any acid, HCl, NHO_3, oxalic, acetic acid, CO_2, he could cause the membrane formation, maturation, normal segmentation, and the formation of normal larvæ in a large percentage of the eggs. In order to produce these results he put the eggs for about five minutes into a mixture of 85 c.c. sea-water plus 15 c.c. *N*/10 acetic acid.

V

These and similar facts may serve us as a basis for the further analysis of the nature of the process of fertilization.

If we call forth the membrane formation in the unfertilized egg of *Strongylocentrotus purpuratus,* either by treating it with benzol or with a fatty acid or with alkali, the same processes take place at first, as in the case of the entrance of a spermatozoon; after some hours a normal nuclear spindle is formed and the nucleus divides regularly into two nuclei. This shows that the synthesis of nuclein salts is started by the membrane formation. If the temperature is very low (from 2° to 5° C.) the segmentation continues slowly but regularly and a few normal blastulæ may be obtained. At a temperature of 15° or more the development does not go beyond the formation of the first nuclear spindle or nuclear division; soon after this the egg begins to disintegrate in a characteristic way. If, however, the egg is put after the membrane formation for from 30 to 50 minutes (at 15° C.) into hypertonic sea-water, all the eggs remain alive and develop, provided the time of exposure is chosen correctly; and in a number of these eggs segmentation and development occur in a normal way. *It is therefore obvious that the calling forth of the membrane formation in the egg starts the nuclein synthesis and the other processes of development, but that the chemical processes do not occur properly.* Through the subsequent treatment of such eggs with hypertonic sea-water these processes are carried back into the proper channels. In some forms, *e. g., Thalassema* and *Asterina,* the calling forth of the process of membrane formation obviously suffices to start the chemical processes in the egg in the right channels and no after-treatment with hypertonic sea-water is required. Our understanding of the developmental effects of the spermatozoon therefore depends upon the answer to the three following questions: (1) What is the physico-chemical character of the process of membrane formation whereby this process is able to start the development of the egg? (2) Why does it start this development in some forms, *e. g., Strongylocentrotus purpuratus,* in the wrong channels? (3) In which way does the treatment of such eggs with hypertonic solution carry the development back into the proper channels? We shall try to answer these three questions in turn.

As far as the physico-chemical character of the process of membrane formation in *Strongylocentrotus* is concerned, we have seen that it can be produced by very different means in the sea-urchin; first, by fat solvents, *e. g.,* benzol, toluol, amylen, etc. Since I had formerly expressed the suggestion that the process of membrane formation might be due to a coagulation and since it might be argued that the above-mentioned agencies might also have a slight coagulating effect, it was of importance to make certain whether they really act only through their fat-dissolving power. Benzol has a high fat-dissolving power and an extremely slight coagulating effect on proteins. Phenol, on the contrary, has a much smaller fat-dissolving power but a very great coagulating effect. If the process of membrane formation were due to a coagulating effect of these agencies, phenol should act much more powerfully in the membrane production than benzol;

if, however, these media act through their fat dissolving power, the reverse should hold. Benzol is practically insoluble in sea-water. For the purpose of the membrane formation about 2 drops of benzol were put into 50 c.c. of sea-water and the mixture shaken. In order to increase the solubility of the benzol in the sea-water the latter was heated slightly. The shaking caused an emulsion, but only a trace of the few drops of benzol went into solution; yet this caused the membrane formation instantly in the eggs of the sea-urchin. On the other hand, phenol is very soluble in sea-water. It was necessary to add 6 c.c. *m*/2 phenol (Kahlbaum) to 50 c.c. sea-water to produce the membrane formation. Moreover, although toluol has been used extensively in experiments on protein solutions, no author has ever noticed a coagulating effect. Yet it is just as effective as benzol for the production of the fertilization membrane. I think there can be no doubt that we are dealing with an action of benzol, amylen, toluol, on the solution of fatty compounds and not on coagulation.

The second agency for the membrane formation is a treatment of the eggs with alkali. The saponifying action of alkalis upon fat is too well known to require any further discussion.

The action of acids, however, is very peculiar and interesting. As already stated, only such acids as contain one (but not more) carboxyl group produce the membrane formation in *Strongylocentrotus purpuratus*. HCl, HNO_3, H_2SO_4, NaH_2PO_4, and dibasic or tribasic organic acids, *e. g.*, oxalic, succinic, citric acids, etc., were practically ineffective. This shows that the effect of the fatty acids can not be due to the hydrogen ion; the hydrogen ion inhibits the process of membrane formation, as can be shown by the fact that the membrane can not be formed as long as the egg is in the acidulated sea-water, but only after it has been transferred back to normal sea-water. Moreover, it can be shown that the ineffectiveness of such acids as HCl, HNO_3, etc., is not due to a secondary injurious effect upon the eggs, for an effective solution of butyric acid remains just as effective if we add to it the equivalent amount of hydrochloric acid. *We are obviously dealing here with a specific action of one group of acids, namely, of those which contain one carboxyl group.* Some of these acids, *e. g.*, acetic, are well-known fat solvents. Pflueger pointed out long ago the fat-dissolving action of oleic acid. All of these monobasic fatty acids are more soluble in fat than the other acids. It is therefore possible that these acids act as fat solvents and that it is due to this action that they cause the membrane formation.

But why should the membrane formation in the egg be connected with the process of fat solution? Several years ago I showed that the process of membrane formation in the egg is a transition stage in such cases of cytolysis of the egg, whereby the latter is transformed into a shadow. If we treat eggs with benzol or amylen they form a membrane and are a few seconds later transformed into shadows. The treatment of the unfertilized eggs with alkali also transforms them rapidly into shadows if the solution is free from Ca or Mg. In this process also a membrane is formed. The treatment of the eggs with a fatty acid does not cause cytolysis, but this is due to the inhibiting action of the H ions. Through the addition of acid to sea-water the cytolytic action of fat solvents like benzol is also inhibited. We can also produce cytolysis by treating the eggs with hypertonic sea-water of a very high osmotic pressure, *e. g.*, 1½ to 2 m., or with very dilute sea-water; in both cases the process of membrane formation is a transitional stage in the cytolysis.

Experiments on cytolysis in red blood corpuscles seem to show that the mechanism of this process is the destruction of the membrane of the red corpuscles chiefly by lipolysis. Koeppe assumes (as is generally agreed) that the surface of the red blood corpuscle consists of a lipoid film which is liquefied, saponified, or otherwise destroyed, in cytolysis. I believe that the same is true for the cytolysis of the egg, with this difference only, that in the egg it is not the most superficial film which is liquefied, but the layer underneath it. The surface film is preserved in this process; it is at first quite thin and invisible, but very soon becomes visible, possibly through an imbibition with water which causes it to swell.

The process of membrane formation, according to these facts, seems to be due to a solution of the fatty layer underneath the surface film of the egg. This fatty layer forms together with the surface film a solid shell around the unfertilized egg. As soon as the fatty layer under the surface film is liquefied, water is squeezed out from the cytoplasm and forms a layer between this and the outside film which in the meanwhile has become toughened. But how could this process of fat solution and possibly lipolysis be connected with the synthesis of nucleins? We can not answer this question except by mentioning the possibility, that the lecithins may be involved in the liquefaction and hydrolysis of the surface layer of the egg.

The second question raised by us was: Why does the process of nuclein synthesis come to a standstill so soon after the membrane formation (unless the egg is treated with hypertonic sea-water) and why does the egg disintegrate so rapidly in this case? To this question we are able to give a pretty definite answer. We stated in the beginning of this paper that processes of oxidation are the *conditio sine qua non* of nuclein synthesis and development in the fertilized egg. The nuclein synthesis and the segmentation of the nucleus and the cytoplasm after the artificial membrane formation also depend upon oxidations and do not occur in the absence of O or the presence of KCN. It can be shown that the disintegration of the eggs does not occur if the eggs are put after the membrane formation into an atmosphere of pure hydrogen, or if the oxidations are suppressed in the egg by the addition of a trace of KCN. Eggs which after the membrane formation are thus treated remain intact and can be caused to develop if after a number of hours they are treated with hypertonic sea-water, while at this time the eggs of the same experiment which had remained in normal sea-water are already disintegrating. We must therefore conclude that the artificial membrane formation causes or allows the oxidations underlying the synthesis of the nucleins, but that these oxidations do not occur in the right direction; and that these faulty oxidations are the cause of the rapid disintegrations of such eggs. This disintegration occurs the sooner the higher the temperature.

This conception receives support through the experiments intended to give an answer to the third question, namely, how it happens that eggs which after the artificial membrane formation are treated for from 30 to 50 minutes with hypertonic sea-water develop normally. It was found in all experiments that a hypertonic solution acts in this way only if it contains free oxygen. If we substitute for the air pure hydrogen or if we add to the hypertonic solution a small amount of KCN this effect is not produced. If the eggs possessing membranes are brought back from the hypertonic solution free from oxygen or containing KCN into normal sea-water, they disintegrate in the same way as if

they had not been treated with the hypertonic solution; if the same eggs are put after the treatment with hypertonic sea-water free from oxygen for from 30 to 50 minutes into hypertonic sea-water containing oxygen they will develop normally when put back into normal sea-water. The dominant rôle of the oxygen in the action of hypertonic sea-water upon the unfertilized egg is still more manifest in experiments on eggs which possess no membranes. If we put the unfertilized eggs of *Strongylocentrotus* directly into hypertonic and hyperalkaline sea-water, *e. g.*, 50 c.c. sea-water plus 10 c.c. 2½NaCl plus 1 c.c. *N*/10 NaHO and leave them in such a solution at 15° C. for about two hours, many eggs will develop after they are transferred back to normal sea-water, while others will be injured and perish in a short time. Both effects, however, are only produced if the hypertonic solution contains oxygen. If it is carefully freed from oxygen or if the oxidations are inhibited by KCN the eggs are intact when taken out of the solution. They will neither develop nor disintegrate when put back into normal sea-water. If after a few hours sperm is added to such eggs they will develop. However one may vary the experiment, the result is always the same, namely that a hypertonic solution stimulates or modifies the development of the egg only in the presence of free oxygen. This seems to indicate that the effect of the hypertonic solution in artificial parthenogenesis consists in a modification of the phenomena of oxidation in the egg; the latter are led back into the right channel. This is the reason why the eggs do not disintegrate but develop if they are treated with hypertonic sea-water after the artificial membrane formation.

VI

If we summarize all the experiments on artificial parthenogenesis it seems that the essential feature of the process of fertilization consists first in a liquefaction or hydrolysis or both, of fatty compounds, and second, in the starting of processes of oxidation in the right direction. In some forms, *e. g.*, *Asterina*, the latter will take place naturally if only the former process is started. In the eggs of many forms the process of liquefaction or saponification of lipoids occurs under the phenomenon of membrane formation. These processes of the liquefaction of fats and hydrolysis and oxidation form apparently the basis of the synthesis of nucleins. It is possible, but far from proved, that among the fatty compounds involved in the process of hydrolysis are the lecithins.

These results are in harmony with the facts observed in the germination of oily seeds. The process of germination is an analogue to the starting of the development in the animal egg, inasmuch as resting cells are thrown into the process of cell division and this process is based upon the synthesis of nucleins. Experiments on the germination of the castor bean have shown, according to Hoyer, that as soon as the seeds are put into water a hydrolytic process is started which results in the formation of acid, chiefly carbonic, lactic, and to a smaller degree, acetic acid. Through these acids a lipolytic enzyme is activated by which the oil of the seed is rapidly hydrolyzed. The rest of the process of germination is primarily a nuclein synthesis. This synthesis depends, as in the case of the egg, upon the presence of free oxygen, since Moritz Traube has shown that seeds can not germinate except in the presence of free oxygen. I think the chemistry of the germination of seeds is essentially the chemistry of nuclein synthesis, and I believe the method of starting this synthesis is essentially the same as in the fertilization of the egg.

We can also understand why certain eggs can develop without fertilization or show natural parthenogenesis, while others require fertilization. The naturally parthenogenetic eggs are those in which the nuclein synthesis can be started without the addition of an outside agency. In analogy with the experience on seeds, we may assume that the acid formed in them after they have left the ovary is sufficient to bring about the necessary hydrolytic process or processes; either directly or through the activation of enzyme. Such eggs must also contain the necessary prerequisite for the normal occurrence of the process of oxidation. In the eggs which require fertilization we must probably discriminate between two groups, one for which the hydrolysis is sufficient to start the nuclein synthesis, *e. g.*, starfish, *Thalassema, Polynoë;* the second group for which in addition provisions are to be made for the processes of oxidation, by treating these eggs with hypertonic sea-water containing oxygen, *e. g.*, sea-urchin, and *Lottia.*

I am of the opinion that this mechanism of nuclein synthesis is the thread by which we can find a rational way through the maze of the otherwise bewildering mechanisms, characteristic of living matter; on one hand, the phenomena of growth, on the other, those of self-preservation.

I will illustrate this by one example. It can be proved that the nucleus itself or one of its constituents acts as a catalyzer in the synthesis of nuclein in the unfertilized egg. This follows from the fact that the velocity of the nuclein synthesis in the fertilized egg increases in proportion with the number of nuclei already present in the egg. If the mass of the original fertilization nucleus is m, the mass of nucleins increases during the first segmentation period to $2m$, during the next to $4m$, and so on, increasing with the exponent of 2; while the duration of the various periods of segmentation differs little and these differences have no relation to the mass of the nuclear material formed during the period. This behavior of a chemical reaction is characteristic for such catalytic processes in which one of the products of the reaction is itself a catalyzer for the reaction. We must therefore conclude that the nuclei themselves or one of their constituents are the catalyzer for the nuclein synthesis or one phase of it. It is possible that the nucleus catalyzes only the phenomena of oxidation, and in as much as oxidations are the conditio sine qua non of nuclein synthesis, this would explain the autocatalytic effect of the nuclei upon this reaction. A number of years ago I pointed out that the nucleus seems to act as the main (though possibly not the only) oxidizing agency of the cell. This influence of the nucleus upon the nuclein synthesis, and the rôle of this synthesis upon the preservation and continuation of living matter, explains one of the most mystifying characteristics of the latter, namely, the phenomenon of automatic reproduction of cells.

JACQUES LOEB
UNIVERSITY OF CALIFORNIA

SCIENTIFIC BOOKS

Introduction to Infinitesimal Analysis: Functions of One Real Variable. By OSWALD VEBLEN and N. J. LENNES. New York, John Wiley & Sons. 1907. Pp. vii + 227. Cloth, $2.00.

Elements of the Infinitesimal Calculus. By G. H. CHANDLER, M.A. Third Edition. New York, John Wiley & Sons. 1907. Pp. vi + 319.

Differential and Integral Calculus with Examples and Applications. By GEORGE A. OSBORNE, S.B. Revised edition. Boston, D. C. Heath & Co. 1907. Pp. xii + 388.

Advanced Algebra. By ARTHUR SCHULTZE, Ph.D. New York, The Macmillan Company. 1906. Pp. xiv + 562.

College Algebra. By Charles H. Ashton and Walter R. Marsh. New York, Charles Scribner's Sons. 1907. Pp. ix + 279.

For more than a century after the inventions of analytical geometry and the calculus, mathematicians and physicists may be said to have fairly rioted in applications of these instruments to geometric, mechanical and physical problems without concerning themselves about the nicer questions of fundamental principles, cogency and precision. The efforts of Euler, Lacroix and others to systematize results served to reveal in a surprising way the need of improving foundations. Constructive work was not arrested by that disclosure. On the contrary, new doctrines continued to spring up and old ones to expand and flourish. But a new spirit began to manifest itself. Mathematics became increasingly critical as its towering edifices more and more challenged attention to their foundations. Already manifest in the work of Gauss and Lagrange, the new tendency, under the powerful impulse and leadership of Cauchy, rapidly developed into a powerful movement. It was the foundations of the calculus that were first overhauled, and, while its instrumental efficacy was greatly improved, the calculus was advanced from the level of a tool to the rank and dignity of a science. Accordingly every genuine university to-day offers two courses in the calculus: an elementary course designed to equip the student with the calculus viewed as an instrument for making rough investigations, and an advanced course designed to acquaint him with the intimate structure of the subtlest of the sciences and to qualify him to use the calculus in the finest and exactest thinking. The work of Messrs. Veblen and Lennes deals with the calculus in the latter conception of it. Their work has but a single English rival, viz., "The Theory of Functions of Real Variables," by Professor James Pierpont, which appeared in 1906. Prior to the appearance of the latter work, an American or English student of the modern critical calculus had to depend upon such foreign works as Jordan's "Cours d'Analyse" and Stolz's "Allgemeine Arithmetik." The aim of such critical work being precision and logical perfection, it tends at first to be prolix and only at last succeeds in becoming concise. The most conspicuous among the merits of the work by Messrs. Veblen and Lennes is the union of conciseness with rigor. This union was effected by means of two principles of economy. One of these is the happy definition of the all-important notion of the limit of a function in terms of the notion of "value approached." The other consists in the systematic employment throughout of a recently established theorem in the modern doctrine of assemblages (ensembles, manifolds, sets), namely, the Borel theorem, so called after its discoverer. The value of the book might be improved by the introduction of more numerous illustrative examples.

The books by Professor Chandler and Professor Osborne, as designed for the beginner, have numerous English rivals. Professor Chandler in this third edition of his book has made some changes to bring the treatment and content into fuller accord with modern demands both of rigor and of utility. The basis is laid in the doctrine of limits. The differential notation is introduced at an early stage, and, everywhere throughout, the reader finds the abstract notions and processes illuminated by simple applications to concrete problems, chiefly of geometry. The closing chapters afford an excellent introduction to differential equations and mechanical integration. Taken all in all, it is one of the more substantial books for the student of engineering, for whom it is primarily designed. It is not one of those emasculated, merely "practician," works that some teachers and students of engineering seem to crave.

By introducing a chapter on series, by rearranging the order of topics, by the earlier geometric illustration of the notion of derivative, and by the incorporation of physical and mechanical applications, Professor Osborne has amply justified the revision of his well-known book, though his decision to give several (not obviously equivalent) definitions of the differential instead of one can hardly fail to annoy the instructor and confuse the pupil.

The book contains too little theory and about three times too many examples. The work demands too much finger work and too little thought.

The algebras by Professor Schultze and by Messrs. Ashton and Marsh cover the material usually presented to high school pupils and college freshmen. Both works are well suited to prepare the student for the examinations held by the College Entrance Examination Board. Messrs. Ashton and Marsh's book begins with the theory of radicals, the preceding matter being presented for review by numerous well-chosen examples. Both works deal admirably with graphs, determinants and the theory of equations. Neither one aspires to the rigor of the superb work of Professor Fine, but both of them are likely to be regarded, whether correctly or not, as more teachable.

C. J. KEYSER

COLUMBIA UNIVERSITY

Metabolism and Practical Medicine. By CARL VON NOORDEN; Anglo-American issue under the editorship of WALKER HALL. Vols. I. and II. Chicago, W. T. Keener & Company.

This work is a translation of the first volume of v. Noorden's "Handbuch der Pathologie des Staffwechsels," the most exhaustive treatise that has yet appeared on the subject of metabolism. The German original (two volumes) is the joint product of the following contributors: v. Noorden, A. Czerny, C. Dapper, Fr. Kraus, O. Loewi, Magnus-Levy, M. Mathes, L. Mohr, C. Neuberg, H. Solomon, Ad. Schmidt, Fr. Steinitz, H. Strauss, and W. Weintraud.

The original of the first volume of the translation comprises 479 pages written by Magnus-Levy. It treats of normal metabolism in all its different phases, and is a very readable piece of metabolism literature. In addition it is a veritable mine of numberless detailed facts and corresponding references to the original literature. It should prove exceedingly valuable to the investigator who wishes to look up definite facts with the minimum waste of time. In some important particulars, as, for example, with regard to the factors which determine the percentage composition of human urine the volume is already more or less obsolete. This is, however, not the fault of the author. His manuscript must have been finished in 1904, and as he says in another connection: "Die Fragen, deren Lösung wir dank der eifrigen Arbeit der verschiedenen Schulen in wohl nicht zu ferner zeit erhoffen dürfen, sind eigentlich zahlreicher, wie die bereits gewonnenen Aufschlüsse und Ergebnisse."

The second volume (of the translation) deals with metabolism in starvation, in overfeeding, in fevers, and in diseases of the digestive tract, respiration, the liver, the blood and the kidneys.

Three of these chapters, namely the first two and the last one, are v. Noorden's, and are written in his usual clear, critical, yet somewhat dogmatic style. These chapters constitute excellent résumés of what is yet known concerning the subjects treated. They abound in concrete instances drawn from the author's experience as a clinician—instances which show how a mastery of even the present limited knowledge of the laws of metabolism is indispensable for the correct diagnosis and the dietetic treatment of many cases coming under the care of every physician.

Of the other chapters in this volume those on fevers (Kraus) and on diseases of the liver (Weintraud) are the most interesting. A part of the matter here introduced, as, for example, Ehrlich's "parallelism" between the process of assimilation and the action of toxins, is perhaps of too hypothetical a nature to merit the extensive discussion it has received. Some of the data presented, notably with regard to the urinary constituents, are of very doubtful value. But taken as a whole these chapters are instructive and suggestive alike to physiologists and to pathologists who are interested in the problems of metabolism.

It is to be regretted that the scholarly character of this valuable work should have suffered at the hands of the translators. They have evidently done their part in great haste, with little regard for English style, and sometimes without even bringing out the correct

sense of the original. A few illustrations of errors which mar the two English volumes may be cited: (Vol. I., p. 3) "the fats and proteins are partly vegetable, partly mineral"; (p. 5) "the questions, the solution of which follow the energetic work of numerous laboratory workers, are really at present more numerous than the discoveries and results obtained";[1] (p. 126) "im Stoffwechsel fast unangreifbar" is rendered as "almost beyond assimilation"; (p. 178) for "genau" we find "minutely"; (p. 403) for "reichlich Getrank zuzuführen" we get "water should be exhibited freely"; (p. 408) for "Kot" we get "motion"; (Vol. II., p. 1) "die Fragen des Chlorumsatzes und der Acetonuria" is transformed into "the problem of the Chlorine changes in acetonuria"; (p. 62) in overfeeding "it is a matter of indifference whether the excess takes the form of albumen or of some oxidizable substance free from nitrogen"; (p. 105) "the nitrogen output during starvation usually exceeds the intake by a small amount.

The German edition was quite up to date when it went through the press (1904–5). A cursory glance at the Anglo-American edition gives the impression that it has carried the review of the literature up to the date of issue. To be sure, some additional data have been incorporated, but much recent literature, especially in the field of normal metabolism, has been omitted.

The translators have cut out a considerable number of references to original literature given in the German edition.

Otto Folin

Plant Physiology and Ecology. By Frederic Edward Clements, Ph.D., Professor of Botany in the University of Minnesota. With 125 illustrations. Pp. xvi + 315. 8vo. New York, Henry Holt and Company. 1907.

This book, the result of more than fifteen years of ecological work on the part of its author, constitutes a notable addition to the literature of botany in America. In 1897 the first edition of Pound and Clements's "Phytogeography of Nebraska" was published, and in it we find the germs of the book now before us. Three years later the second edition appeared with considerable emendations and corrections, and this was followed by various papers published in magazines, transactions of societies and university "studies," culminating in Clements's "Research Methods in Ecology" in 1905, a technical handbook for the advanced student and investigator of ecology. The book in hand is based upon that work, and is, in fact, a less technical presentation of the same matter, adapted to the needs and preparation of college and university students.

The author considers physiology and ecology as essentially the same, hence the treatment is in accord with this view. The author does not in any place give the plant an ecological consideration as distinct from one which is physiological. It makes no difference whether the plant is studied in the laboratory, or in its field or forest habitat, in any case the investigation is essentially the same, and we are no longer to call the first physiology and the second ecology.

The book is broken up into fifteen chapters, of which the first is a somewhat philosophical discussion of stimulus and response. And here we get the author's definition of plant physiology in this paragraph (page 1): "Physiology was originally understood to be an inquiry into the origin and nature of plants. This is the view that pervades the following pages, and in accordance with this the subject-matter of ecology is merged with that of physiology." The nature of stimuli and the nature of response are discussed—concisely and precisely—and adjustment and adaptation are defined and delimited. Then follow chapters on the water of the habitat, adjustment to water (including absorption, transport and transpiration), adjustment to light, adjustment to temperature, adjustment to gravity, contact and shock, adaptation to water, and adaptation to light. In these chapters, along with much discussion of the problems involved, the au-

[1] This sentence purports to be a translation of the German sentence of Magnus-Levy's quoted above.

thor introduces fifty-seven practical tests for their experimental demonstration. Some of these experiments are familiar to every student of plant physiology, while others are new, and in many cases quite novel. Some of them are to be performed in the laboratory, while others take the student out into the fields and forests.

The ninth chapter, on the origin of new forms, is again a philosophical presentation, including a summary discussion of the law of evolution, stability and plasticity, constant and inconstant forms, origin by adaptation, origin by variation, origin by mutation, natural selection, isolation, polygenesis, etc. Several instructive pages are given to Darwin and his predecessors and followers.

The remaining chapters include methods of studying vegetation, the plant formation, aggregation and migration, competition and ecesis, invasion and succession, alternation and succession. Even in these chapters some experimental work is suggested, so that the student will not depend wholly upon observation and camera-pictures for his conclusions! It is safe to say that the student who learns his ecology in the way it is presented in this book will not do as much guessing at his facts, and drawing of inferences from landscape photographs, as has been the habit of some of the "ecologists" of the immediate past.

CHARLES E. BESSEY
THE UNIVERSITY OF NEBRASKA

SCIENTIFIC JOURNALS AND ARTICLES

The Journal of Comparative Neurology and Psychology for September contains two articles on animal behavior. Dr. C. H. Turner writes on "The Homing of Ants: An Experimental Study of Ant Behavior," concluding from an extensive series of field and laboratory experiments that ants find their way to and from the nest neither by tropisms nor by a homing instinct, but that they learn their way by experience. The elements which enter into this experience were subjected to experimental analysis. The second paper is by Dr. E. H. Harper, on "The Behavior of the Phantom Larvæ of *Corethra plumicornis* Fabricius." These larvæ have a very characteristic mode of locomotion in the water. They conform neither to the conventional mode of orientation laid down in the tropism scheme nor to the trial and error type of reaction, but rather to a unique type of reaction system of the larva.

THE last number of *Symons's Meteorological Magazine* contains the following note: "The five hundredth number of *Symons's Meteorological Magazine* is now before our readers, a fact of no little interest when the smallness of the public to which such a journal appeals is taken into account. When Mr. Symons produced No. 1 in February, 1866, he had already issued a "monthly rain circular," as a supplement to "British Rainfall" for several years, so that a greater antiquity might plausibly be claimed for the magazine than the numeral implies. The magazine, though small, has grown, and is not, we trust, incapable of further growth without departing from the original lines on which it was planned. As an independent organ of opinion in meteorological matters, it has, we believe, been of use in the past, and we hope that this usefulness will continue. We heartily thank the many friends who have helped us hitherto, and we look forward with confidence to a wider circle of readers.

DISCUSSION AND CORRESPONDENCE

THE PARASITISM OF NEOCOSMOSPORA

IN SCIENCE for September 13, 1907, Dr. Erwin F. Smith, of the Bureau of Plant Industry, U. S. Department of Agriculture, makes certain criticisms on work which the writer published some time ago in a bulletin of the Missouri Agricultural Experiment Station and in a note in SCIENCE.

My purpose in writing the papers mentioned was to record in permanent form observations which I had made in course of a study of the ginseng fungus. I submitted some conclusions which it seemed proper to draw, because there has been more or less disagreement on the parasitism of these fungi among mycologists.

Dr. Smith seems to think that the ginseng fungus is not identical with the watermelon fungus, although he himself does not appear to have done any work upon it. I am perfectly willing to admit that the two fungi may not prove to be identical, and will bear no personal ill will to any one who may establish the facts. There are, however, certain points in the communication mentioned on which a few words may be said at this time.

He doubts my identification of the ginseng fungus and criticizes me for not making "any comparative study of the two fungi, although it would have been easy" to obtain the melon fungus. I think that the following letter gives *one* very good reason why I was unable to compare the ginseng fungus with the wilt fungus he studied.

U. S DEPARTMENT OF AGRICULTURE,
Bureau of Plant Industry
Vegetable Pathological and Physiological Investigations
Laboratory of Plant Pathology.

WASHINGTON, D. C., Jan. 31, 1905

MR. HOWARD S. REED,
University of Missouri,
Columbia, Missouri

Dear Sir: In looking over my snowed-under desk yesterday I found your letter of October 27, and am very much afraid it was unanswered. I regret to say that I have no cultures of the fungus which you wish, to wit, *Neocosmospora*, described in Bulletin 17 of the Div. of Veg. Phys. and Path. I have not worked at all on the disease now for a long time and allowed all the cultures to die. It would really take all of one person's time, and perhaps rather more, to keep going in good condition cultures of all the things that we work with, and it occasionally happens that one or another dies, or is lost for the time. If I come across it again, I will try to keep in mind that you wish a culture.

Yours very truly,
(Sig.) ERWIN F. SMITH,
In Charge of Laboratory of
Plant Pathology

The fact that I did not find perithecia seems to impel a particularly sharp shaft of criticism at me. In this connection the reader will permit me to refer to the bulletin written by Dr. Smith, mentioned in the foregoing letter (p. 10).

> . . . the conidial stage of the watermelon fungus (spore taken in July from the interior of a vessel) has been cultivated for five years on a great variety of media, including potato, without showing a trace of perithecia, although from time to time special efforts were made to find a substratum which would lead to the production of perithecia. This is the strain of fungus which has proved so actively parasitic in the hands of the writer.

He states again (p. 11):

> No perithecia ever developed in any of the cultures made from internal or external conidia taken from the cotton or watermelon.

Having only the conidial stages to start with, it is not surprising that I did not obtain the perithecial stages.

In his haste to criticize my work he appears to have fallen into the same grievous heresy of which he accuses me. From what I said concerning the parasitism of *one* species of the form-genus *Fusarium*, he recklessly gained the impression that I had made sweeping statements concerning the parasitism of that entire genus. If he will take the trouble of again reading my note in SCIENCE[1] he will find that the closing sentence *especially restricts* my statements concerning weak parasitism to the form I isolated from the ginseng plant. After carefully reexamining the text of the bulletin and also the note, I find nothing which conveys the impressions which he has apparently gained. This seems to be an "unwarranted inference" in his "course in logic."

It is a matter of no little satisfaction that in this last communication he has reported the outcome of inoculation experiments by a colleague in which sterile soil was used. In his earlier work he used "good earth" but does not appear to have taken the very important precaution of excluding all other organisms from the soil. Considering the prevalence of other soil fungi, it is surprising that he should have neglected to insure

[1] Vol. XXIII., p. 751.

sterile conditions in his own work, and this was the point on which I based certain criticisms in the publication cited. It seems to me that this is the kind of work Dr. Smith "should have done and not left for some one else to do."

HOWARD S. REED

September 20, 1907

AN OFFICIAL LETTER ON "TEMPERANCE PHYSIOLOGIES"

THE circular letter below was recently received by certain publishing firms in New York City. While it was intended for the *guidance* of publishers, it will certainly be suggestive to educators who are interested in freedom in science teaching. Some comment follows the letter.

DEAR SIRS:

It is probably well known to you that the National Woman's Christian Temperance Union, through the department of Scientific Temperance Instruction, Mrs. Mary H. Hunt, former superintendent, has been active in securing the publication of good school physiologies and their introduction into the schools, and that heretofore satisfactory books have borne the printed announcement that they had been endorsed, such endorsement being signed by Mrs. Mary H. Hunt, or by some member of the Advisory Board as appointee of the National W. C. T. U.

You are also probably aware of the fact that Mrs. Hunt and the W. C. T. U. have been repeatedly accused of receiving royalties on "endorsed" physiologies. Having been assured by Mrs. Hunt that no such royalties were received by her, we have for years unhesitatingly and unreservedly denied that any royalties were received by the W. C. T. U. or by any representative of the organization. If any one has received any royalties on endorsed physiologies, such receipt of monies was wholly unauthorized by the W. C. T. U., was positively against its policy, and was never reported to the W. C. T. U.

Since the death of Mrs. Hunt our organization has considered very carefully the present situation and has decided upon the following policy:

1. The National W. C. T. U. will continue to encourage the publication of new series of physiologies to replace weak or old series now in use, and urgently requests that all good series be revised frequently, that they may be kept up to date in all scientific and pedagogical points.

2. We shall encourage the direct relation of authors and publishers, exerting our influence to help publishers to find thoroughly competent authors and to help such authors to find good publishers.

3. We shall request publishers of school physiologies to publish a note in the preface of each book giving the name of some well-known specialist in physiology who has approved the book as to its scientific accuracy, especially concerning the latest deductions of science on the alcohol question, and that of some well-known educator who has approved the book as to its pedagogy.

Our world's and national superintendent of the Department of Scientific Temperance Instruction in Schools and Colleges and director of the Bureau of Scientific Temperance Investigation is Mrs. Edith Smith Davis, 2913 Brown St., Milwaukee, Wisconsin, a woman admirably fitted to lead this great department of our work. Mrs. Davis will have associated with her a number of well-known men in the scientific and educational world as counsellors of the department. Any scientists or educators acceptable to Mrs. Davis and her counsellors whose endorsement you may secure for your books will ensure the hearty cooperation of the National W. C. T. U. in the circulation of said books. Any physiology which fails to meet a high and satisfactory standard on the question of the effect of alcohol and narcotics on the human system will be publicly disapproved by the National W. C. T. U. and our local unions over the entire country will work against the introduction of such books into the schools.

We shall show no partiality between publishers of satisfactory text-books and will continue to do all we can to secure the teaching of physiology and hygiene in the schools, and to secure the introduction of good books.

Yours for the presentation of unbiased truth, on behalf of the General Officers of the National W. C. T. U.

This letter was signed by the president and secretary of the National W. C. T. U.

The letter has the following interesting points:

1. The question of royalties said to have been received is one of minor importance to scientific educators. Certainly no one ever believed that the W. C. T. U. as an organization could ever have descended to vote approval for accepting "royalties."

2. Scientific teachers are glad to know that publishers will be "encouraged to replace weak

or old books"; but they must wonder what society is charged with the duty of "encouraging" publishers to keep other books up to date. Should we not have a society expressly devoted to the stimulation of lazy authors and especially publishers? Any readers of the weekly lists of new books and new editions will readily see that the easy-going New York publishers need to be reminded that they must keep their books "up to date in all scientific and pedagogical points." Those of us who are interested in physiology and hygiene for schools are so altruistic that we want somebody to "encourage" the authors and publishers to keep other kinds of books up to date.

3. The note of approval by "some well-known specialist in physiology and some well-known educator" is a vast improvement over the former "endorsement" by a committee among whose members there were no well-known specialists in physiology or education. At first the change looks hopeful, for many books published with the old endorsement would never be approved by any well-known specialist in physiology who was also an expert in public school education. But any hopes of a new order of things which may be raised by paragraph 3 in the letter above are dissipated by the next paragraph, in which publishers are definitely informed that "any scientists or educators acceptable to Mrs. Davis and her counsellors" may be considered "well-known specialists in physiology" competent to write an approving preface for new text-books. This *may* work satisfactorily in practise; but before becoming too optimistic we want to see the list of specialists who might be "acceptable" to Mrs. Davis. How many members of the American Society of Physiologists, the American Society of Zoologists, and the American Society of Naturalists will be on the "acceptable" list, unless they first pledge themselves to views also "acceptable"? Will members of these societies be able without special instructions to judge concerning the "high and satisfactory standard" so that publishers may be sure of avoiding the financial loss and prestige which will follow "public disapproval"? It seems clear that publishers must make some careful diplomatic moves before they venture to print a manuscript under the advice of "well-known specialists in physiology." They may save themselves a lot of trouble by first getting a list of the "acceptable" men of science of the first rank. It can not be long enough to be cumbersome for office use.

The letter above is worth reading carefully. It indicates that the old order of things in "temperance physiology" still attempts to continue. Probably most readers of this journal believe in "presentation of the unbiased truth" concerning alcohol and narcotics and as teachers would insist upon having books which tell the essential truth so far as it has been demonstrated; but few indeed must be the readers who do not recognize the brazen effrontery of the letter above. Between the lines it reads that our well-known specialists in physiology and our educators are such incompetents that their books and even their written approval of books by others must not be published before they have been adjudged sane, satisfactory and acceptable. Truly this is an interesting footnote to the most astounding chapter in the history of American education.

M. A. Bigelow

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THE TYPES OF THE NORTH AMERICAN GENERA OF BIRDS[1]

Much of the chaos in generic nomenclature which has become intolerable to the systematist of to-day has been brought about by the failure of many writers to explain by what process they have determined the types of old polytypic genera. Had they been more explicit upon this subject we should have been able long ago to see the weaknesses in our codes and should have abandoned methods which were neither definite nor final in their operations.

The recent paper by Dr. J. A. Allen on the "Types of the North American Genera of

[1] *Bulletin American Museum Natural History*, Vol. XXIII., Article XVI., pp. 279–384, April, 1907.

Birds" is therefore especially welcome, as it throws a flood of light upon a hitherto obscure subject and shows us the possibilities of the various methods of determining genotypes. The presentation in concise form of the original basis upon which each genus was erected will be exceedingly useful to the systematic ornithologist, but Dr. Allen's conclusions with regard to the types of many of the older polytypic genera will hardly be accepted since he has not followed any one method consistently in his determinations; and consistency is absolutely essential in work of this sort. He tells us that "from the standpoint of elimination" his results agree with those given in the A. O. U. check list in all but four instances.

A careful study of his text, however, shows that he uses "elimination" in a surprisingly broad sense and that his types are really variously determined by "elimination"; "subsequent designation"; "general consent," and "restriction." Any one of which used throughout would have materially increased the number of changes necessary in the check list. Frequently two methods are applied to the same case with different results and Dr. Allen accepts one method in one instance and another in the next.

For example,

Pelecanus, p. 301.

Type designated by Gray, 1840—*onocrotalus*.

Type by elimination—*aquila*.

Conurus, p. 335.

Type designated by Gray, 1841—*leucophthalmus*.

Type by elimination—*carolinensis*.

Gray's designation is accepted by Dr. Allen in the first instance and elimination in the second, and, in both, his types agree with the check list. Obviously such choice is unwarranted; one method or the other must be used throughout.

In other cases only one method is mentioned while another method would give a different result, viz.:

Passerina, p. 357.

Type by elimination—*nivalis*.

Alca, p. 293.

Type by elimination—*torda*.

In both these cases Gray designated other species as type in 1840, but his designations so frequently accepted by Dr. Allen in preference to elimination are here completely ignored.

Many of Dr. Allen's eliminations, moreover, would yield entirely different results, were all the involved genera considered. For example:

glacialis, given as the type of *Fulmarus* by elimination, was removed from that genus as the type of *Rhantistes*, 1829, and

garrulus, given as type of *Ampelis* by elimination, was removed as the type of *Bombycivora*, 1815.

And so in many other cases critical genera are omitted from consideration.

The recent action of the American Ornithologists' Union and the Nomenclature Commission of the International Zoological Congress in repudiating the elimination method has probably forever removed this "bone of contention" from consideration.

There are left, as stated recently by President Jordan, two methods of type fixing, either of which will yield definite and final results—the first species rule and type by subsequent designation. By either method some fourteen changes will have to be made in the genera and subgenera of the A. O. U. check list, and in determining just what they are Dr. Allen's paper will be a valuable aid.

In this connection and with the idea of increasing its usefulness it seems desirable to call attention to some errors in quotation, etc.—inevitable in a work of this scope. The type of *Daption* will be found to be designated in the original publication, while that of *Thalassens* designated by Gray in 1840 was *cantiaca*, not *caspia*. Gray, moreover, did not designate a type for *Herodias* in 1840, and in 1841 selected *garzetta*, not *egretta*. The type of *Anous* which he selected in 1840 was not *Sterna niger* Stephens (= *S. stolida* L.) but *S. nigra* Linn. (= *Hydrochelidon nigra*). The genera *Dendragapus*, *Hydranassa* and *Cistothorus* are polytypic, not monotypic, as indicated.

Dr. Allen gives 415 instead of 435 as the

number of genera and subgenera in the check list, but this is evidently a pure *lapsus*, as only eleven are omitted in his paper, viz., *Endomychura, Cymochorea, Phœbastria, Palassicarbo, Rhyacophilus, Uranomitra, Burrica, Chelidonaria, Pachysylvia, Myiobius* and *Neocorys*.

WITMER STONE

SPECIAL ARTICLES

SOME OLD-WORLD TYPES OF INSECTS IN THE MIOCENE OF COLORADO

THE work of the past summer at Florissant has yielded us a large collection of fossils from the Miocene shales. Most of the material awaits examination; but a few things of unusual interest have been examined, and these have been found to include some forms allied rather to those of the Old World than to those now inhabiting this continent. A brief notice of these is now given. The specimens themselves were exhibited at the recent Zoological Congress in Boston.

DIPTERA

Glossina oligocena (Scudder)

A very good specimen, found at Station 14 by Mr. Geo. N. Rohwer. The mouth-parts are preserved, as also the body, wings and legs, all agreeing so well with the modern tsetse flies that generic separation is impracticable. The genus *Glossina* is to-day confined to Africa, and although placed by Austen in the Muscidæ, is a very peculiar type, better regarded as belonging to a distinct family Glossinidæ. The fossil species is not new, but was described by Scudder as *Palæstrus oligocenus* in 1892,—a supposed new genus of Œstridæ. Scudder's type, which has been compared with the new specimen, lacked the head and other important parts, otherwise its true position would certainly have been recognized.

The former existence of a tsetse fly in America is of particular interest as having a possible connection with the disappearance of some of the Tertiary Mammalia, as Professor Osborn had suggested.

HYMENOPTERA

Perga coloradensis sp. nov.

A good specimen from Station 14 (W. P. Cockerell). A large sawfly, about 27 mm. long and very robust; referable to the Australian genus *Perga*, and similar to *P. schiodtii* Westwood, from New South Wales, but larger, the antennæ longer and with a larger club (length of club, 3 mm.), stigma much more slender, virtually rudimentary, an interval of more than 1 mm. between the basal and cubital nervures at their place of approximation, and the scutellum and prothorax dark like the rest of the thorax. The anterior wing is 20 mm. long, and the basal nervure meets the transverso-medial. Konow makes the tribe Syzygoniides to include two Australian genera (with thirty-seven species between them) and two Brazilian genera (with three species between them). The fossil is clearly of the Australian, not the Brazilian, type, suggesting that the route of migration was a northern one, *via* Asia.

NEUROPTERA

Halter americana sp. nov.

A wonderfully preserved example with the wings spread, from Station 13 B (S. A. Rohwer). The anterior wings are clear hyaline, 31 mm. long, with the venation as usual in the genus; hind wings (as in all the Nemopteridæ, to which family it belongs) very long and narrow, length 46 mm., with an apical fiddle-shaped expansion, which is dark colored. The Nemopteridæ are to-day confined to the Old World, except a single species of *Stenorrhachus* found in Chile. The Florissant insect is not of the Chilian genus, but belongs to that section of *Halter* which includes the Persian *H. extensa* (Oliv.). In *H. extensa* the black area of the hind wings is broken into two, whereas in the fossil it is solid and continuous. The persistence of such an extremely peculiar type through such a long time and such migrations indicates a remarkable degree of stability.

Panorpa arctiiformis sp. nov.

Station 14 (W. P. Cockerell). A spotted species, looking like an Arctiid moth; wings about 13 mm. long. Close to *P. rigida* Scudder, already described from Florissant, but larger, with the third band (the last before the dark apex) much broader. Among the living

species, it resembles *P. nuptialis,* Gerst., from Texas, and *P. picta,* Hagen, from Smyrna; but it is closer to the latter. In *nuptialis* the inner edge of the apical black area is straight, but in *picta* it is zigzag; in the fossil *arctiiformis* it is as in *picta.*

One of the new plants obtained this year in the Florissant Miocene is of interest in the same connection. It is *Heyderia coloradensis* sp. nov., an incense cedar very closely allied to the living *H. decurrens* (Torrey) K. Koch, of California. The larger leaves are about 5½ mm. long, perhaps less acutely pointed than in the living plant, but in arrangement and general structure quite the same. I follow Dr. N. L. Britton in separating *Heyderia* from the *Libocedrus* of the southern hemisphere; the genus contains two living species, one in California, the other Asiatic. Our fossil agrees very closely with a specimen in the herbarium of the New York Botanical Garden which was collected by Torrey in 1865 in the grove of *Sequoia gigantea* in California. As showing the persistence of a "plant association," it is interesting to note that the fossil species also grew by or under *Sequoia* trees, as is proved by a fragment of *Sequoia haydeni* on the same piece of shale, touching the *Heyderia. Sequoia haydeni,* the Florissant redwood, is the *Hypnum haydeni* of Lesquereux, a fragment of it having been originally described as a moss!

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UNIVERSITY OF COLORADO,
BOULDER, COLORADO,
September 1, 1907

CENSUS OF FOUR SQUARE FEET

THE approximate numbers of the fruits and seeds, the insects and other invertebrates present on a given area are data for which the student of economic problems has frequent need. It is important, therefore, to have a more accurate conception of the abundance of these items than is derivable from offhand estimates. The present paper is offered as a contribution to the knowledge of this subject, with the explanation that the results herein detailed are not held applicable to any classes of surface other than those examined, nor to any region but that of Washington, where the collections were made. However, it is probable they safely may be used as a basis for reasonable analogies with respect to other localities.

Pausing to note only that the investigation was undertaken primarily because of its relation to the study of bird food, the method used was as follows: areas, two by two feet, of forest floor and of grassy meadow, were examined in November and March, respectively. Everything on the surface of these plots and the ground itself, to the depth a bird easily can scratch, was removed and all plant and animal objects of classes known to be used as food by birds were counted. The following were obtained from the four square feet in the woods: Coleoptera 12, Hemiptera 7, Hymenoptera 8, spiders 11, other Arthropods 26, Annelids 9, Gasteropods 11, cocoons and insect eggs 27, or altogether 112 animal items; in addition there were 194 seeds and fruits. Assuming, as we may in perfect justice, that the plot in question was in no way exceptional, the analysis indicates that on the average there are present on each acre of forest floor in this locality 1,216,880 animals of the kinds above specified and 2,107,810 fruits and seeds.

From the four square feet of meadow there were collected: Coleoptera 61, Hemiptera 20, Hymenoptera 940, spiders 53, other Arthropods 127, Annelids 33, Gasteropods 20, cocoons and eggs 20, or altogether 1,254 animal objects; there were also 3,113 seeds. The averages per acre for meadows, therefore, are: Animals, 13,654,710, and seeds, 33,822,745. On first thought these estimates seem incredible. Hence in order to show that they are the result of the summing up of numbers, individually so small, that no one would question them, I present the following complete lists[1] of the living invertebrates and seeds from each area.

WOODS: INVERTEBRATES: *Chilopoda* 7; *Oligochæta* 9; *Thysanura* (including 1 *Japyx* sp.) 15; *Homoptera* (*Gypona* sp.) 2; *Heteroptera* (*Euschistus fissilis* 2, *E. tristigmus* 1, *Cryphula parallelogramma* 1, *Rhyparochromus* n. sp. 1) 5; *Coleop-*

[1] For the greater part of the identifications of insects the writer is indebted to Messrs. E. A. Schwarz, O. N. Heidemann and Nathan Banks.

tera (*Arthmius globicollis* 6, *Ctenistes piceus* 2, *Sunius prolixus* 1, *Glyphonyx recticollis* 1, *Telephorus* sp. (larva) 1, *Odontota* n. sp. 1) 12; *Lepidoptera* (caterpillars) 2; *Hymenoptera* (*Ponera pennsylvanica* 1, *Aphænogaster aqua* 1, *Lasius americanus* 6) 8; *Pseudoscorpionida* 1; *Araneida* (*Xysticus transversus* 1, *Ceratinella emertoni* 1, *Anyphæna saltabunda* 1, *Oribata angusticeps* 1, *Philodromus ornatus* 1, *Synemosyna formica* 1, *Ceratinopsis interpres* 1, *Chthonius pennsylvanicus* 1, *Fuentes vittata* 1, *Lycosa pratensis* 1, *Theridula sphærula* 1) 11; *Acarida* 1; *Gastropoda* 11. Total, 85. Cocoons and Eggs: *Orthoptera* (eggs of *Diapheromera femorata*) 2; *Lepidoptera* (cocoons) 6; *Diptera* (Puparia) 6; *Araneida* (egg sacs) 4; unidentified: 3 eggs; 4 cocoons. Total, 27. Grand total, invertebrates, cocoons and eggs, 112. Seeds: *Juniperus virginiana* 9; *Panicum* sp. 89; *Carex* sp. 1; *Quercus* sp. 2; *Liriodendron tulipifera* 57; *Rubus* sp. 7; *Chamæcrista fasciculatis* 6; *Trifolium repens* 4; other Leguminosæ 17; *Viola* sp. 2; *Cornus florida* 1. Total, 194.

MEADOW: Invertebrates: *Chilopoda* 4; *Oligochæta* 33; *Thysanura* 102; *Homoptera* (*Liburnia* sp. 4, *Agallia 4-maculata* 8, *Tettigoniidæ* 3, *Goniognathus palmeri* 1) 16; *Heteroptera* (*Corimelæna atra* 3, *Capsidæ* 1) 4; *Coleoptera* (*Pterostichus lucublandus* 2, *Stenolophus conjunctus* 1, *Cryptobium pallipes* 1, *Scopæus opacus* 11, *Stilicus rudis* 1, *Erchomus læviusculus* 5, *Falagria venustus* 1, *Apocellus sphæricollis* 6, *Euæsthethus americanus* 1, *Philonthus thoracicus* 2, *Tachyporus jocosus* 1, *Diochus schaumii* 6, *Mycetoporus flavicollis* 2, *Trichopteryx haldemani* 6, *Melanophthalma americana* 2, Lampyridæ (larva) 1, Elateridæ (larvæ) 3, *Tomoderus constrictus* 7, *Sitones hispidulus* 1, (unidentified larva 1) 61; *Lepidoptera* (caterpillars) 6; *Hymenoptera* (*Tetramorium cæspitum* 933, *Lasius* sp. 2, *Formica* sp. 1, *Chalcididæ* 4) 940; *Araneida* 53; *Acarida* 15; *Gastropoda* 20. Total, 1,254. Cocoons and Eggs: *Heteroptera* (Pentatomid eggs) 2; *Orthoptera* (grasshopper eggs) 4; other insect eggs 2; unidentified pupa cases 12. Total, 20. Grand total, invertebrates, cocoons and eggs, 1,374. Seeds: *Andropogon* sp. 46; *Panicum* sp. 39; *Chætochloa viridis* 138; *Eragrostis* sp. 8; *Eulalia zebrina* and other grass seed 2,783; *Allium vineale* (bulblets) 3; *Rumex obtusifolius* 2; *Amaranthus* sp. 4; *Portulaca oleracea* 1; *Pyrus* sp. 1; *Chamæcrista nictitans* 5; *Trifolium repens* 3; *T. pratense* 1; *T.* sp. (sprouting) 18; *Robinia pseudacacia* 6; *Vicia* sp. 1; *Euphorbia maculata* 4; *Vitis* sp. 1; *Verbena urticæfolia* 3; *Paulownia tomentosa* 1; *Ambrosia artemisiæfolia* 1, *Bidens bipinnata* 40; *Sonchus asper* 1; unidentified 3. Total, 3,113.

The most interesting point about the estimates, for the woods and the meadows, aside from their astounding magnitude (contrasted to previous figures based wholly on hypothetical grounds), is the wide discrepancy between them. The population of the meadow is so much more dense than that of the woods. Weighing the items separately, it is easily seen which contribute most largely to the discrepancy. Of the very great number of Hymenoptera (940) found in the pasture, 933 were of one species of ant (the cosmopolitan *Tetramorium cæspitum*). Yet there was no ant colony on the plot; hence direct evidence is lacking that the number found is exceptional. Only 8 ants, however, were found in the forest. Further, the meadow yielded 102 spring tails to 15 in the woods where the decaying leaf mold seems the optimum home. Turning to seeds, it must be admitted that 2,716 of the 3,113 seeds found in the meadow were of a single gramineous species, but this hardly can be termed extraordinary since an abundance of grass seeds must be expected in a meadow. However, to be entirely on the safe side let us subtract these strongly contrasting elements from the totals above stated. Still there remain 397 seeds and 239 animals for the meadow plot, an average of more than double the number secured in the woods.

This result is in harmony with many of the conspicuous phenomena of sylvan and campestran life. Witness the variety and copiousness of the vegetation of roadsides and meadows; their wide expanse of flowers attracts a humming swarm of insects. Enter the deep forest, comparative monotony attends and 'tis quiet as the tomb. As one progresses through the more open woods to the meadows again, step by step, variety and number increase. In no respect are phenomena of this sort better exemplified than in the relative density of the bird population. It is a matter of repeated observation that birds are more abundant in open country, but it has remained for Professor S. A. Forbes first to give the matter definite expression

founded on careful investigation. In his valuable paper of April, 1907, "An Ornithological Cross Section of Illinois in Autumn," the account of a trans-state survey in which 4,804 birds were counted, it is stated that the number of birds per square mile of woods was 785, and in pastures 1,551. While a mere coincidence, yet it is of interest that these figures bear the same relation to each other as do the numbers of the principal food elements.

Another point of interest in connection with the close investigation of the surface fauna relates to dead insects. Insects number so many, that even if we admit, as we do in the case of other animals, the death of each is a tragedy, it stands to reason that this tragedy can not in every case be enacted by captor and prey. Many must die of other causes and simply fall to the ground. What becomes of them? Some are eaten by sarcophagous insects, but are any to be found? The question was raised in the writer's mind by the finding of some suspiciously old and apparently weathered fragments of insects in bird stomachs (segments of Millipeds in the mourning dove, a practically entirely vegetarian species, and of adult June bugs in winter stomachs of some other birds). Were not these possibly picked up as fragments? It is now the writer's opinion that this is more than possible.

At any rate there is no lack of dead insects to be picked up by any bird desiring them. On the plot of forest floor were found nine dead invertebrates entire, and the fragmentary remains of 36. Fewer of such remains were found in the meadow, perhaps because the multitude of ants disposed of a large proportion of them. But even here there were 8 intact bodies and 14 broken. On the basis of these figures there are tangible remains of 240,030 departed insects on each acre of meadow and 488,925 on each acre of woods. Of both the living and the dead there are a host, but the dead of ages reduced to dust are insignificant beside the living of a single season.

W. L. McATEE.

QUOTATIONS

AGRICULTURAL EDUCATION

To one very important condition of success both advocates and opponents of *la petite culture* in England pay, we suspect, too little regard—namely, the improvement of agricultural education, for the heads as well as for the rank and file of the industry. In too many of our country districts it is hardly yet realized that education is necessary at all. Landowner and tenant-farmer are alike disposed to lay blame for the rural exodus on such education as is given to the laboring classes—an education which, it may be admitted, has not always been best adapted to fit them for a country life and pursuits. But they forget that education is, after all, but an incident of the great social and economic changes that have come over English life in the past half century; and that, if all our elementary schools could be restricted to-morrow to teaching "the three R's" and all boys sent out to farm work at ten or eleven years of age, there would still remain the daily newspaper, the bicycle, and the excursion train to give the laborer that wide outlook and "progressive desire" which is what really draws him away from the land. So far from there being, as Squire Oldacre and Farmer Hodge are apt to think, too much education already, what is needed is much more of it, but of a different kind; education in the elementary school that will bear directly on country life and inspire some taste for it; education continued afterwards in evening schools or technical instruction classes to widen the knowledge and sharpen the wits of those who are to cultivate the soil, and to instil into them at least the beginnings of scientific method. The day of rule-of-thumb is over, in agricultural as in other industries; the day of science—that is, of trained and organized knowledge—has begun, and the nation or class that despises it must fall behind. It is not undue treatment in freight charges, or unpatriotic preference for foreign goods, that enables the small Danish butter-farmer, for instance, to undersell the Englishman on his own markets, but superior education and scientific method applied

to the organization of his industry; and we may be sure of this, that it will be useless to keep a man on the land, or to bring him back to it, by the inducement of ownership or any other attraction, unless we can educate him to do the best for himself and for the land, in an age which calls for cultivated intelligence and scientific method.—The London *Times*.

CURRENT NOTES ON LAND FORMS

RELATION OF VALLEYS TO JOINTS

THERE have been various articles published on the relation of drainage lines to joints, involving a problem of which the gorge of the Minnehaha below its falls may be taken as an example. It is sometimes pointed out that the course of this gorge has been determined by the arrangement of the joints in the horizontal rock series through which the gorge has been cut. There can be little question that the process of weathering has taken advantage of the joints in the widening of the gorge, and that its walls exhibit joint faces more or less frequently; but it is also evident that the gorge has been cut backward along the course that the stream had on the drift cover up-stream from the falls; and that the relation of the gorge to the joints is therefore one of accidental superposition, and not of submissive guidance.

On the other hand, it is often plain that a group of master joints may guide the development of a new (subsequent) branch stream which grows by headward erosion in the bare walls of a young valley; for in such cases there is no stream falling into the valley from the upland above. Subsequent streams of this origin may be common in some districts; but if they pass into maturity, they will probably wander on their flood plains so as to depart more or less from the guiding joints beneath the valley floor; and if afterwards rejuvenated by elevation, it is eminently possible that they may stray away from the joints that originally led them. The chance of such straying will increase with the lateness of the stage at which the first cycle of erosion is interrupted by rejuvenating elevation.

VALLEYS OF SOUTHWESTERN WISCONSIN

E. C. HARDER presents a discussion of part of this problem in a thesis entitled "The joint system in the rocks of southwestern Wisconsin and its relation to the drainage network" (Bull. Univ. Wisc., no. 138; Science series, iii., 1906, 207–246). He first determined by numerous observations the dominant joint directions in a certain part of the Wisconsin driftless area; he next determined, apparently from maps, the dominant stream directions of the same district. Then he compared these two sets of dominant directions, independently determined, and finds that "the prominent directions of jointing correspond with the prominent drainage directions" (p. 232). "Many other forces [than joints] may have been present to modify the result," but their influence is thought to have been small (p. 234). Further investigation is looked to for additional results.

In all cases of this kind, in which the more or less precise coincidence in the directions of a large number of lines is the chief guide to the conclusion, several critical questions arise. First, what are the limits of error in the determination of the measured directions, and what are the limits of discordance in cases that are classed as coincidences? Second, what are the possibilities of coincidence by chance instead of by causal relation? Third, is the conclusion that a causal relation exists between the two sets of lines whose directions coincide, supported by independent evidence that the supposed cause can produce the inferred effect? Fourth, are other causes shown to be inoperative?

Joint directions are determinable easily within small limits of error; but stream directions are much less easily determined, because stream lines are as a rule so irregularly curved. Moreover the curved streams of the mature valleys of driftless southwestern Wisconsin demonstrably depart to-day from their earlier courses to a greater or less extent; and it is therefore not clear whether the present or the earlier courses are to be regarded as joint controlled. Some close analysis, with a quantitative statement as to the percentage of total

stream length that is reducible to definite directions and with a statement of the amount in tabulating the measures, is here much needed. A quantitative discussion of coincidence is also desirable.

On the second point, there is opportunity for a mathematical discussion of the theory of chances, for there may, of course, be a considerable number of accidental agreements in two sets of directions such as are here considered. On the third point, it is only reasonable to ask that joints should be shown competent to determine the direction of the drainage lines under consideration, for if they possess no such competence, then all the coincidences of direction would have to be attributed to chance. The problem here appears to be briefly as follows: Given an ancient land area, subject to normal processes of erosion in many successive (incomplete) cycles of erosion; in what way can the originally consequent drainage be influenced by underground joints? It can hardly be supposed that the joints were at the beginning of such power as to have determined the course of the original drainage—that would be possible only if they were gaping fissures or faults. The original drainage was presumably consequent on the original slopes and inequalities of the land surface. As to streams of later development, it is possibly the case that in a district of resistant rocks, rapidly given great altitude, the headward growth of subsequent streams along joints in the walls of the consequent stream valleys might in time replace the minor consequents; but we have at present no sufficient inductive basis for such a conclusion; perhaps the case brought forward by Iddings may be of this kind, but there the argument of coincidence has not been supplemented by any argument of competence. On the other hand, a region of relatively weak rocks, slowly elevated to a moderate altitude—and this is a supposition appropriate to the Wisconsin district under consideration—the opportunity for the growth of subsequent streams along joint lines would be very poor; the valley sides would usually be cloaked with waste, so that the capacity of the joints to guide the development of new streams would be small as compared with the capacity of the existing streams to persist in their courses. There remains the possible tendency of streams to change their courses by lateral shifting so as to come into relation with underground joint systems; but on this point we have little information. Finally there is the possibility of other causes than joints for stream directions in the district studied. Some other causes certainly have operated in some instances, for there are various streams indicated on Harder's maps, unrelated to neighboring joint directions (for example, Underwoods creek, near Avoca, Pl. III.). In this connection there should be a critical examination of the stage of development of each stream; for while a stream in a vertical-walled gorge might well be closely related to a joint system, it is quite otherwise with a stream that is swinging on the flood plain of an incised meandering valley. Streams of the latter class appear to occur not infrequently in the district studied, but no special mention is made of them in the text: surely the coincidence of an intermeander tangent with a joint direction can mean little; for the tangent has in all probability changed its direction and its location during incision, and these changes have been almost inevitably controlled by the changes in the adjacent meanders rather than by underground joints.

In all these respects additional study is needed before demonstration can be reached. The fact of frequent coincidences, if well attested, is therefore a good first step in an inquiry which we hope Harder may continue to follow.

BLOCK MOUNTAINS IN NEW ZEALAND

THE south-central part of the southern island of New Zealand possesses a group of block mountains, "probably not less remarkable than the ranges of the Great Basin between the Sierra Nevada and the Wasatch," of which a brief description is given by J. Parks, professor of mining in the University of Otago ("The Geology of the Area covered by the Alexandra Sheet, Central Otago Division," N. Z. Geol. Survey, Bull. 2, N. S.,

1906). The older rocks of the district are chiefly deformed mica schists. They have been reduced to a peneplain, which is now uplifted and scored in its seaward portion by deep, narrow valleys. Farther inland, the peneplain has been broken into a number of sub-parallel blocks, of which the higher ones form mountain ranges, generally trending northeast-southwest, and reaching from 5,000 to 6,000 feet altitude; while the relatively depressed blocks are buried under heavy basin deposits of lacustrine, fluviatile and glacial origin. The ranges are not forested and expose a large amount of bare rock; they are as a rule flat topped; "the skyline of the Hawkduns for a distance of twenty miles appears as even as the ridge of a house"; they have no lateral spurs or foothills, but descend to an almost straight base line. The basin deposits have been more or less terraced and dissected. A rough cross-profile suggests that some of the heaved blocks are bounded by faults on both sides, and that, though retaining old forms on their flat uplands, they have lost much of their edges by new erosion.

The few pages from which the above statements are gleaned are only incidental to the chapters on geological and economic problems to which the body of the report is devoted; the student of physiography must, however, be grateful if he gets even so much as these few pages from a professor of mining, whose first responsibilities are elsewhere directed. Yet is it not probable that, if a well-systematized plan for the physiographic description of block mountains were in general use among geographers, there would not so often be occasion to regret that significant physiographic features are passed over without mention in a geological report? To illustrate: When a comet is discovered, it is customary for astronomers to secure at once certain previously planned observations, according to a standardized method; and from these observations the elements of the comet's orbit are promptly calculated according to an accepted scheme. As a result, the chief items which astronomers have come to agree upon as essential regarding comets are immediately determined and placed on record. No such standardized method of procedure is yet adopted by geographers, still less by explorers; and as a result it is largely a matter of chance whether the description of a new member of an already known class of land forms—block mountains, for example—will include its essential elements or not. In a case of the kind above cited, the most important elements would probably be: the general structure of the region, briefly stated at first, with details added later when needed; the stage of erosion that the region had reached when block-faulting took place; the relation of the fault lines to the preexistent structural lines and topographical features; the number and attitude of the heaved and of the thrown blocks; systematic account of the main drainage lines, sufficient to show whether they persist from the previous cycle as antecedent streams in spite of the adverse faulting, whether they are revived into new activity by favoring deformation, whether they are of a new generation consequent upon the slope of the tilted or faulted faces of the displaced blocks, or whether they are developed as subsequent streams by headward erosion along newly exposed weak structures; definite indication of the stage reached in the work of erosion by the several kinds of streams on their valley lines, and in the work of gradation by weathering on the fault faces and valley sides, with particular reference to the manner in which the features produced in the new cycle are related to those still holding over from the previous cycle and to those produced directly by faulting; indication of the stage reached in the work of aggradation (and afterwards of degradation) over the thrown blocks; and finally, as many specific details as possible, not described empirically, but systematically presented as instances of the above-named elements.

It will always be difficult for even the best trained physiographers to secure a complete record of all desired elements of land forms; but the knowledge of land physiography will be immensely advanced when work in the field is carried on in view of carefully standardized and generally accepted schemes,

and when published reports are so phrased as to make explicit reference of observed items to their proper class, and explicit statement that such and such elements were not determined. W. M. D.

JAMES CARROLL

MAJOR JAMES CARROLL, Surgeon U. S. A., died at his home in the city of Washington on September 16, after an illness of about seven months.

Major Carroll was born in England June 5, 1854. When about fifteen years of age he emigrated to this country, and on June 9, 1874, enlisted in the United States army, and served as private, corporal, sergeant and hospital steward from that date to May 21, 1898, when he was appointed an acting assistant surgeon.

While still a soldier he began the study of medicine at the University of the City of New York during the session 1886–7. After a break of a year he resumed his medical studies in Baltimore at the University of Maryland, 1889–91, and received his degree from that institution.

He was appointed first lieutenant, assistant surgeon, in the medical corps, October 27, 1902, and promoted to the grade of major-surgeon, by special act of congress March 2, 1907, on account of his services in connection with the discovery of the mode of transmission of yellow fever, and the courage shown by him in subjecting himself to experiment with a view to demonstrating the method of transmission by a mosquito.

Doctor Carroll's was the first experimental case of yellow fever. He suffered a very severe attack to which he attributed a heart trouble, which finally caused his death.

Doctor Carroll's interest in the subject of yellow fever did not cease with the discovery of the method of its transmission, but he continued to make many independent contributions to the literature on the subject.

The Havana Yellow Fever Commission, appointed upon the recommendation of Surgeon General Sternberg, U. S. A., in 1900, consisted of Major Walter Reed, Surgeon, U. S. A.; Dr. James Carroll, Dr. Jesse W. Lazear and Dr. A. Agramonte. The death of Dr. Carroll leaves Dr. Agramonte, a Cuban physician, as the only surviving member of the commission.

The bacteriological and experimental investigations of the commission were to a large extent conducted by Dr. Carroll. During Major Reed's absence in the United States the inoculations by means of infected mosquitoes were begun.

On August 11, 1900, Dr. Lazear made the first experiment, but nine distinct inoculations on persons, including himself and Acting Assistant Surgeon A. S. Pinto, were unsuccessful. We know now that these failures were due to two facts—first, that patients after the third day of the disease can not convey the infection to the mosquito, and second, that after having bitten a yellow-fever case the mosquito can not transmit the disease until after an interval of at least twelve days. On August 27 a mosquito was applied to Dr. Carroll which happened to fulfil both of these conditions. The result was a very severe attack of yellow fever in which for a time his life hung in the balance.

G. M. S.

SCIENTIFIC NOTES AND NEWS

THE Silliman lectures by Professor William Bateson will be given in the Peabody Museum at Yale University on October 8 and the following days. The subject of the course is "The Problems of Genetics."

THE Herter Lectures before the Medical Department of the Johns Hopkins University will be given this session by Edward A. Schäfer, LL.D., F.R.S., professor of physiology in the University of Edinburgh, at the end of April, 1908. The Turnbull Lectures on poetry will be delivered by Professor A. V. Williams Jackson, of Columbia University, on Persian Poetry, probably in February.

PROFESSOR OTTO PFLEIDERER, of the University of Berlin, began a series of six lectures in German on "The German Philosophy of Religion," at Harvard University, on September 30.

The Hon. James Mackintosh Bell, director of the New Zealand Geological Survey, will lecture at Harvard University on October 11 on "Travel in Little Explored Parts of New Zealand."

Dr. W. McM. Woodworth, of the Museum of Comparative Zoology of Harvard University, has undertaken to edit the proceedings of the Seventh International Zoological Congress.

The London *Times* states that the Flückiger gold medal has been awarded to Professor Edouard Heckel, the director of the Colonial Institute at Marseilles. The medal was founded by Dr. F. A. Flückiger, of Strasburg, in 1893, and is awarded every five years, in recognition of steps taken to promote the advancement of scientific pharmacy, irrespective of nationality. Mr. E. M. Holmes, the curator of the museums of the Pharmaceutical Society of Great Britain, received the first medal, and the second was presented to Dr. C. Schmidt, of Marburg.

Captain James M. Phalen and Lieutenant Henry J. Nichols, assistant surgeons, U. S. Army, have been appointed members of the army board for the investigation of tropical diseases, replacing Captains Percy Ashburn and Charles F. Craig, assistant surgeons, who have been ordered to return home, their tour of duty on foreign service having expired.

Dr. Harold L. Lyon has resigned his position as assistant professor of botany at the University of Minnesota to accept a position as assistant director of the pathological laboratory on the experiment station maintained by the Hawaiian Sugar Planters' Association, at Honolulu.

Mr. Homer D. House has resigned the associate professorship of botany and bacteriology in Clemson College, South Carolina, and will spend the coming year at the New York Botanical Garden.

Mr. Edward C. Johnson, formerly assistant in botany at the University of Minnesota, has been appointed assistant pathologist in the Department of Agriculture.

Professor Henry S. Munroe, head of the department of mining of Columbia University, has returned, after a five-months' trip in South America. He spent most of his time in Bolivia.

Professor Charles Harrington, of the Harvard Medical School, attended the Congress of Hygiene and Demography, which met at Berlin at the close of September.

Dr. James G. Hardy, professor of mathematics at Williams College, and Dr. H. F. Clelland, professor of geology, have been given leave of absence for the second half of the present academic year.

The autumn lectures at the New York Botanical Garden will be delivered in the lecture hall of the museum building of the garden, Bronx Park, on Saturday afternoons, at 4 o'clock, as follows:

October 5—"The Salton Sea and its Effect on Vegetation," by Dr. D. T. MacDougal.

October 12—"Collecting Fungi in the Wilds of Maine," by Dr. W. A. Murrill.

October 19—"The Forms and Functions of Leaves," by Dr. C. Stuart Gager.

October 26—"The True Grasses and their Uses," by Mr. George V. Nash.

November 2—"The Giant Trees of California: their Past History and Present Condition," by Dr. Arthur Hollick.

November 9—"The Progress of the Development of the New York Botanical Garden," by Dr. N. L. Britton.

November 16—"Edible Roots of the United States," by Dr. H. H. Rusby.

Professor Leveson Francis Vernon Harcourt, emeritus professor of civil engineering at University College, London, died on September 14, at the age of sixty-eight years.

The U. S. Civil Service Commission announces an examination on October 16–17, to fill vacancies as they may occur in the position of scientific assistant in the Department of Agriculture, at salaries ranging from $840 to $2,000 per annum, depending upon the experience and qualifications of appointees. As a result of this examination certification will be made to fill two vacancies in the position of scientific assistant in rural engineering (road

making), Department of Agriculture, at $840 per annum. The department also desires to secure one or two scientific assistants in animal husbandry who are qualified in the special branch of horse industry.

THE British government has made an additional grant of $2,500 a year to the Liverpool School of Tropical Medicine, making $5,000 a year for five years.

A PASTEUR INSTITUTE was opened at the University of Minnesota in August, and has been under the charge of Dr. Orinna McDaniel. Seventeen persons have received treatment for threatened rabies.

IN a note on additions to the Zoological Garden, the London *Times* states that in point of importance, from a scientific point of view, three young Chinese alligators (*Alligator sinensis*) undoubtedly head the list. Up to 1879 it was generally believed that alligators were confined to the New World, though in 1870 Swinhoe established the existence of a saurian in the Yang-tsze-kiang, which he described as "a young crocodile about 4 feet long." Nine years later, however, M. Fauvel, a French official of the Chinese Customs, published an account of the animal, which proved to be an alligator, closely allied to the well-known species of the Southeastern United States, known as the Mississippi alligator, from which, however, it differs considerably in its much smaller size. The coloration of the upper parts is greenish black, with yellowish vermiculations, and greyish below. According to M. Fauvel this reptile was the origin of the mythical dragon of the Chinese. In 1880 stuffed specimens reached this country; two living examples were received at the Zoological Gardens in 1890, of which one is still alive, and two were exhibited at Frankfort-on-the-Main in the same year. From that time, however, no living examples appear to have been imported till now. The gayal herd in the cattlesheds has been increased by the birth of a calf, which is of good augury, for before the arrival of the small herd in April last these animals had not been represented in the collection for some time. A young clouded leopard (*Felis nebulosa*) has been received and placed in the small mammals' house. Although usually reckoned among the larger cats, the name "tiger," formerly applied to this animal, conveys an erroneous impression, for its size does not exceed that of a small leopard. There is a good deal of variation in the ground-color of the fur, which ranges in different individuals from greyish to yellowish brown, fading into white on the under-surface. The new arrival belongs to the dark form, and is said to have come from Sumatra. Two rare monkeys have been received and placed in the insect house. One is the red-faced ouakari (*Brachyurus rubicundus*), an entirely arboreal species from the region of the Amazon; it is of small size, with a short tail. The fur is reddish-brown in hue, and the face a deep red, as is suggested in the popular name. A red-eared guenon (*Cercopithecus erythrotus*) may be readily distinguished by the color of the inside of the ears, and perhaps more readily by the red nose-spot.

UNIVERSITY AND EDUCATIONAL NEWS

THE state legislature has appropriated for the Michigan College of Mines, at Houghton, $75,000 for a library and museum building and $43,000 for a new central heating and power plant.

FOLWELL HALL, erected for the University of Minnesota at a cost of $415,000 for the work of the College of Science, Literature and Arts, was opened at the beginning of the present academic year. The new building for the agricultural department, erected at a cost of $250,000, was opened during the summer.

MRS. W. G. FARLOW has given $1,000 to Radcliffe College, Harvard University, the interest of which is to be used for the purchase of scientific and mathematical books for the library.

THE Lowell Institute, in cooperation with Harvard University, will offer during the current academic year two free courses of lectures corresponding closely in subject matter, methods of instruction, examinations and

scale of marking with History 1 and English *A* as given in Harvard College. The lectures will be given in the buildings of the Harvard Medical School at 8 P.M.

Professor Alfred C. Cole, of the Ohio State University, has been elected professor of physics at Vassar College, succeeding Professor LeRoy C. Cooley, who has retired at the age of seventy-four years, after thirty-three years' service.

At Wellesley College, Dr. Carl M. Wiegand, of Cornell University, has been appointed associate professor of botany, and Dr. Daniel Starch, who is on leave of absence from the Ohio State University, has been appointed acting instructor in psychology.

Dr. H. E. Wells has recently resigned the professorship of chemistry at Allegheny College to accept a similar position at Washington and Jefferson College. Professor Edwin Lee, of Mount Union College, has been appointed to fill the vacancy thus left open at Allegheny College.

At Bowdoin College Mr. Ralph B. Stone has been appointed instructor in physics and mathematics, and Mr. H. M. Hastings, instructor in descriptive geometry.

Dr. Hiram Bingham has been appointed instructor in South American geography in Yale University.

Changes in the staff of the botanical department of the Ohio State University for the current year are as follows: Professor Schaffner has been given leave of absence for the year to study in Germany, and Dr. A. Dachnowski (Michigan) has been appointed as substitute. Professor Griggs has spent the summer at Port Renfew Minnesota Botanical Station, studying the Kelps. Miss Detmers, instructor, botanized in California and Alaska until September 15. Dr. Kellerman has been given leave of absence for the winter term to conduct a peripatetic School of Botany in Guatemala.

The following elections and promotions have been made at the Iowa State College for the year 1907–8: *Division of Agriculture*—J. A. McLean and Wayne Dinsmore have been promoted to associate professorships of animal husbandry; J. B. Davidson to a professorship of agricultural engineering; E. B. Watson to an assistant professorship of soils, and John Bower to an assistant professorship of dairying. *Division of Veterinary Medicine*—R. R. Dykstra has been promoted to an associate professorship of veterinary medicine, and C. H. Stange and W. E. Madson to assistant professorships. *Division of Science*—W. F. Coover has been promoted to an associate professorship of chemistry; Mrs. Alice Dynes-Feuling has been appointed professor of domestic science, and Miss Helen Donovan has been promoted to an associate professorship and Miss Ruth Morrison to an assistant professorship; Alexander S. Thompson has been appointed director of music, and Mrs. A. S. Thompson, instructor of voice. *Division of Engineering*—W. H. Meeker has been promoted to a professorship of mechanical engineering, W. M. Wilson to an associate professorship, and R. A. Norman has been appointed assistant professor; J. E. Kirkum has been appointed associate professor of civil engineering and H. C. Ford, assistant professor; L. C. Hodson has been promoted to an associate professorship of mining engineering and I. A. Williams to an associate professorship; F. A. Fish has been promoted to an associate professorship of electrical engineering, and W. B. Anderson and A. H. Hoffman have been appointed assistant professors.

The following changes have occurred in the teaching and station force of the biological department of the North Dakota Agricultural College and Experiment Station: Mr. F. J. Pritchard, assistant professor of botany, has resigned to take up advanced work in plant breeding at Cornell University. Mr. F. J. Seaver, late fellowship student in Columbia University and graduate of Iowa State University, has been elected to fill the vacancy. Mr. T. D. Beckwith, of the Department of Agriculture, Division of Water Purification Investigations, has been elected assistant professor of bacteriology and plant pathology, and will be associated with Professor Bolley in special soil investigations in the experiment station work.